

IV. *Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, Bart. K. B. P.R.S.*

Read January 24, 1805.

MY DEAR SIR,

IT is well known that the fluid, generally called the Sap in trees, ascends in the spring and summer from their roots, and that in the autumn and winter it is not, in any considerable quantity, found in them; and I have observed in a former Paper, that this fluid rises wholly through the alburnum, or sap-wood. But DU HAMEL and subsequent naturalists have proved, that trees contain another kind of sap, which they have called the true, or peculiar juice, or sap of the plant. Whence this fluid originates does not appear to have been agreed by naturalists; but I have offered some facts to prove that it is generated by the leaf;* and that it differs from the common aqueous sap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which DU HAMEL has called the *suc propre*, and which I will call the true sap,) the whole substance, which is annually added to the tree, is derived. I shall endeavour in the present Paper to prove that this fluid, in an inspissated state, or some concrete matter deposited by it, exists during

* See Phil. Trans. of 1801, page 336.

the winter in the alburnum, and that from this fluid, or substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring; and thus furnishes those organs, which were not wanted during the winter, but which are essential to the further progress of vegetation.

Few persons at all conversant with timber are ignorant, that the alburnum, or sap-wood of trees, which are felled in the autumn or winter, is much superior in quality to that of other trees of the same species, which are suffered to stand till the spring, or summer: it is at once more firm and tenacious in its texture, and more durable. This superiority in winter-felled wood has been generally attributed to the absence of the sap at that season; but the appearance and qualities of the wood seem more justly to warrant the conclusion, that some substance has been added to, instead of taken from it, and many circumstances induced me to suspect that this substance is generated, and deposited within it, in the preceding summer and autumn.

DU HAMEL has remarked, and is evidently puzzled with the circumstance, that trees perspire more in the month of August, when the leaves are full grown, and when the annual shoots have ceased to elongate, than at any earlier period; and we cannot suppose the powers of vegetation to be thus actively employed, but in the execution of some very important operation. Bulbous and tuberous roots are almost wholly generated after the leaves and stems of the plants, to which they belong, have attained their full growth; and I have constantly found, in my practice as a farmer, that the produce of my meadows has been immensely increased when the herbage of the preceding

year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the summer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the summer, in the preparation of matter calculated to afford food to the expanding buds and blossoms of the succeeding spring, and to enter into the composition of new organs of assimilation.

If the preceding hypothesis be well founded, we may expect to find that some change will gradually take place in the qualities of the aqueous sap of trees during its ascent in the spring ; and that any given portion of winter-felled wood will at the same time possess a greater degree of specific gravity, and yield a larger quantity of extractive matter, than the same quantity of wood which has been felled in the spring or in the early part of the summer. To ascertain these points I made the experiments, an account of which I have now the honour to lay before you.

As early in the last spring as the sap had risen in the sycamore and birch, I made incisions into the trunks of those trees, some close to the ground, and others at the elevation of seven feet, and I readily obtained from each incision as much sap as I wanted. Ascertaining the specific gravity of the sap of each tree, obtained at the different elevations, I found that of the sap of the sycamore with very little variation, in different trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter ; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free

from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or

less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age, and where many, of the same size and growing with equal vigour, spring from the same stool. One half of the poles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being left on all, and all being placed in the same situation to dry.

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

This difference of ten *per cent.* was considerably more than

I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.583 , and that of the summer-felled to be 0.533 ; and trying the same experiment with similar pieces of wood, but taken from poles which had grown on a different stool, the specific gravity of the winter-felled wood was 0.588 , and that of the summer-felled 0.534 .

It is evident that the whole of the preceding difference in the specific gravity of the winter and summer-felled wood might have arisen from a greater degree of contraction in the former kind, whilst drying; I therefore proceeded to ascertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when steeped in water. Having therefore reduced to small fragments 1000 grains of each kind, I poured on each portion six ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had sunk to 60° , I found that the winter-felled wood had communicated a much deeper colour to the

water in which it had been infused, and had raised its specific gravity to 1.002. The specific gravity of the water in which the summer-felled wood had, in the same manner, been infused was 1.001. The wood in all the preceding cases was taken from the upper parts of the poles, about eight feet from the ground.

Having observed, in the preceding experiments, that the sap of the sycamore became specifically lighter when it had continued to flow during several days from the same incision, I concluded that the alburnum in the vicinity of such incision had been deprived of a larger portion of its concrete or inspissated sap than in other parts of the same tree: and I therefore suspected that I should find similar effects to have been produced by the young annual shoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain less extractive matter than an equal portion taken from the lower parts of the same pole, where no annual shoots or leaves had been produced.

No information could in this case be derived from the difference in the specific gravity of the wood; because the substance of every tree is most dense and solid in the lower parts of its trunk: and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces several equal portions of wood taken from different parts of the same poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in others that from the upper parts of the poles,

had given to the water the deepest colour and greatest degree of specific gravity ; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring : and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes ; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases during several successive years : it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear-tree, at a small distance from the trunk ; and I pared off, at the same time, the whole of the lifeless external bark. The age of this tree, I have good reasons to believe, somewhat exceeded two centuries : its extremities were generally dead ; and it afforded few leaves, and no fruit ; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer : but in the beginning of July

numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those, which the tree had born in the three preceding seasons; and I cannot believe that the matter which composed these buds and leaves could have been wholly prepared by the feeble vegetation and scanty foliage of the preceding year.

But whether the substance which is found in the alburnum of winter-felled trees, and which disappears in part in the spring and early part of the summer, be generated in one or in several preceding years, there seem to be strong grounds of probability, that this substance enters into the composition of the leaf: for we have abundant reason to believe that this organ is the principal agent of assimilation; and scarcely any thing can be more contrary to every conclusion we should draw from analogical reasoning and comparison of the vegetable with the animal economy, or in itself more improbable, than that the leaf, or any other organ, should singly prepare and assimilate immediately from the crude aqueous sap, that matter which composes itself.

It has been contended* that the buds themselves contain the nutriment necessary for the minute unfolding leaves: but trees possess a power to reproduce their buds, and the matter necessary to form these buds must evidently be derived from some other source: nor does it appear probable that the young leaves very soon enter on this office: for the experiments of

* THOMSON's Chemistry.

INGENHOUZ prove that their action on the air which surrounds them is very essentially different from that of full grown leaves. It is true that buds in many instances will vegetate, and produce trees, when a very small portion only of alburnum remains attached to them; but the first efforts of vegetation in such buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this case, no grounds to suppose that the leaves derive their first nutriment from the crude sap.

It is also generally admitted, from the experiments of BONNET and DU HAMEL, which I have repeated with the same result, that in the cotyledons of the seed is deposited a quantity of nutriment for the bud, which every seed contains; and though no vessels can be traced* which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous passage, which is perfectly similar to that through which I conceive the sap to be carried from the leaves to the buds, in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

We have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms, on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only,

* HEDWIG.

as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the matter prepared and deposited in the preceding year; and hence the root becomes exhausted and spoiled: and HASSENFRATZ found that the leaves and flowers and roots of such plants afforded no more carbon than he had proved to exist in bulbous roots of the same weight, whose leaves and flowers had never expanded.

As the leaves and flowers of the hyacinth, in the preceding case, derived their matter from the bulb, it appears extremely probable that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many species precede their leaves: and, as the roots of plants become weakened and apparently exhausted, when they have afforded nutriment to a crop of seed, we may suspect that a tree, which has borne much fruit in one season, becomes in a similar way exhausted, and incapable of affording proper nutriment to a crop in the succeeding year. And I am much inclined to believe that were the wood of a tree in this state accurately weighed, it would be found specifically lighter than that of a similar tree, which had not afforded nutriment to fruit or blossoms, in the preceding year, or years.

If it be admitted that the substance which enters into the composition of the first leaves in the spring is derived from matter which has undergone some previous preparation within the plant, (and I am at a loss to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at least,) it must also be admitted that the leaves which are generated in the summer derive their substance from a similar source; and this cannot be conceded without a direct admission of the

existence of vegetable circulation, which is denied by so many eminent naturalists. I have not, however, found in their writings a single fact to disprove its existence, nor any great weight in their arguments, except those drawn from two important errors in the admirable works of HALES and DU HAMEL, which I have noticed in a former memoir. I shall therefore proceed to point out the channels, through which I conceive the circulating fluids to pass.

When a seed is deposited in the ground, or otherwise exposed to a proper degree of heat and moisture, and exposure to air, water is absorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every subsequent stage of the growth of the root, it increases in length by the addition of new parts to its apex, or point, and not by any general distension of its vessels and fibres; and the experiments of BONNET and DU HAMEL leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured infusions, when its point was taken off, and I was totally unable to discover any alburnous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but

when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former Paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into and possibly give existence to, other leaves; and thus by a repetition of the same process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

But it has been proved by DU HAMEL that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The

vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude that when the true sap has been delivered from the cotyledon or leaf into the returning, or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of

sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through the leaf in the preceding year ; but that it is generated by a process similar to that of the germination of seeds, and that the same process is always going forward during the spring and summer, as long as the tree continues to generate new organs. But towards the conclusion of the summer I conceive that the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter.

I have some reasons to believe that the true sap descends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability. If subsequent experiments prove that the true sap descends through the alburnum, it will be easy to point out the cause why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted : and why some portion of alburnous matter is in all trees* generated below incisions through the bark.

It was my intention this year to have troubled you with some observations on the reproduction of the buds and roots of trees;

* I have in a former paper stated that the perpendicular shoots of the vine form an exception. I spoke on the authority of numerous experiments ; but they had been made late in the summer ; and on repeating the same experiments at an earlier period, I found the result in conformity with my experiments on other trees.

but as the subject of the Paper, which I have now the honour to address to you, appeared to be of more importance, I have deferred those observations to a future opportunity ; and I shall at present only observe, that I conceive myself to be in possession of facts to prove that both buds and roots originate from the alburnous substance of plants, and not, as is, I believe, generally supposed, from the bark.

I am, &c.

Elton, Dec. 4,
1804.

T. ANDREW KNIGHT.